

But when viewed from the standpoint of soil chemistry, possibly a correlation may be found in the apparent diversity of habitats. Dr. Edgar T. Wherry tells me that, in the case of two stations in Vermont which he was able to test, the soil in which the species grew gave a moderately acid reaction. He believes that the soil of the wooded slopes of the Delaware River would give a similar reaction, and suggests that even the Springdale station, near limestone cliffs, would probably be of an acid character from the humus in which the plant grows, while the Clementon locality would undoubtedly be acid—and probably of a high grade of acidity. It may thus at least be suggested that, until conclusive work has been done on the species along this line of research, *B. matricariaefolium* probably shows a preference for acid soils.

ACADEMY OF NATURAL SCIENCES OF PHILADELPHIA

Is *Botrychium dissectum* a Sterile Mutant?

The accompanying illustration of a plant of *Botrychium dissectum* bearing no less than three well-developed fruiting panicles would seem to answer the above question emphatically in the negative so far as sterility is concerned. Certainly, this individual is doing all that could reasonably be expected of it to avoid that condition. Normally, of course, the *Botrychium*s of the *tornatum* group fork once near the ground, one branch bearing a sterile and the other a fertile segment. In this case, the forking has been repeated higher up and a secondary fertile branch given off which has itself divided just above its base producing two secondary fruiting panicles somewhat smaller than the primary one. Such compound forms are occasionally found in *obliquum*; I have a specimen collected at South Windsor, Conn., by C. W. Vibert in which the branching exactly

matches that in the picture. A hasty and far from complete search in the literature at hand discloses two records of similar forms in *dissectum*, one reported by Mrs. Scoullar (Fern Bulletin 16: 85), the other by Mr. Poyser (Fern Bulletin 17: 68).

The specimen illustrated was found by Mr. C. C. Deam in Cass Co., Indiana, in Sept., 1921, among the colony of *dissectum* described by him in the last number of the Journal, and we are indebted to him for the photograph and for generously defraying the cost of the plate.

To return to the original question, my own experience strongly supports Prof. Hopkins's opinion (Fern Journal 11: 114) that had Prof. Chamberlain extended his observations over a wider territory, he would have come to quite different conclusions. Indeed, I am confident that had he looked in any large herbarium he would have found plenty of specimens of *B. dissectum* with well developed sporangia full of spores quite as perfect in appearance as those of *obliquum*. In my own small collection, I have six sheets of fruiting *obliquum* and two of *dissectum*, the latter both of a type sufficiently extreme so that the question of identity raised by Prof. Hopkins does not concern them. If they are not *dissectum* there is no such thing. The size of the sporangia varies considerably, even on the same plant. The largest happen to be on a specimen of *obliquum*, but so are the smallest. Those of *dissectum* fall well within the limits of variation in *obliquum*. There are some shrunken and apparently abortive sporangia on one specimen of *dissectum*; but so there are on one of *obliquum*. *Dissectum* has numerous spores which, under the microscope, appear normal and viable, and are of the same size as those of *obliquum* and in no way different from them.

Among the hundreds of living plants I have seen in the field, I have never noticed any tendency to sterility in *dissectum*. To be sure, I was not looking for it, and one can overlook a good deal when one's mind is not bent on that particular thing. But it seems unlikely that any general tendency toward sterility in *dissectum* would have escaped the observation of the many botanists in the northeastern states who know it well in the field.

Mr. Deam has collected a series of small plants of *dissectum*, and in 1909 Prof. Schnaffner reported in the *Ohio Naturalist* (Vol. 10, p. 8) the finding of prothallia which were just putting forth their first tiny leaves; and these leaves showed the *dissectum* character. There is, of course, no absolute proof that these young plants sprang from spores, but it seems most probable that they did; and since at Mr. Deam's locality, a great deal of *dissectum* and only one plant of *obliquum* were found, the probability in that case becomes almost a certainty. I am quite ready to believe that *dissectum* is only a form of *obliquum*; I should not be surprised if the same plant were found to produce both types of leaves at different times, as happens in the case of the incised form of the Christmas fern. But I do not believe that *dissectum* is generally incapable of producing good spores.

In regard to the point made by Dr. Benedict (*Fern Journal* 11: 54), that sterility is often associated with lacinate leaf-forms, it may be noted that the species he cites are not dimorphic like our *Botrychium*. Would lacination of the leaf affect the fruiting segment in dimorphic forms? In the Christmas fern which is partially dimorphic, the incised form is commonly more heavily fruited than the typical form. In this species the incised form often, though not always, appears as a response to unfavorable conditions of growth, as when the soil is dried out by the cutting off of woods. Under such circumstances plants are likely to produce unusual

quantities of seeds or spores, as it were in a supreme effort to insure the perpetuation of the species. But the lacinate forms of cultivated ferns are rather associated with abundant food and excess of vegetative vigor—conditions which always tend to produce sterility. The analogy between *B. dissectum* and the incised form of the Christmas fern is not perfect—the former, for instance, never, so far as I know, shows any evidence of being a response to environment—but it is perhaps closer than the other.—C. A. W.

On a Supposed Hybrid in Equisetum.

C. A. WEATHERBY.

There are certain groups in our flora in which species, otherwise good and generally recognized as such, are connected by puzzling intermediate forms, sometimes in a very complicated manner. In recent years a good deal of work has been directed toward explaining these forms as hybrids of the species between which they fall. Much of this work is highly valuable: it furnishes, not only a reasonable and in several cases practically proved explanation of conditions as we find them, but a means of classification at once elastic and definite enough for all practical purposes. This is very good; but when, as occasionally happens, I find a plant not uncommon in a given region determined as a hybrid between two species, one or both of which are unknown there, I begin to feel that it is possible to have too much of this good thing—that maybe theories of hybridization offer rather too easy a means of disposing of puzzling forms.

The immediate occasion of this discourse is a recent experience of mine. In 1915 Miss Ruth Holden published a study of *Equisetum variegatum* var. *Jesupi*¹ in which, from the facts that its spores were apparently

¹ Holden, Ruth, The Anatomy of a hybrid Equisetum. Am. Journ. Bot. 2: 225-233, pls. 5-8. Aug., 1915.